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REVIEW

KEVIN KELLY
The Logic of Reliable Inquiry
Oxford, Oxford University Press, 1996

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This is a lively and controversial book that attempts to defend at great length an extravagant thesis: the central concept of probability, the most important methodological concept in standard accounts of inductive scientific inquiry, should be replaced by the concept of computability. This is not the first time that this thesis has been defended. Perhaps the most important place to find a stout-hearted defence is in the learning-theoretical literature in the book, *Systems That Learn*, by D. Osherson, M. Stob, and S. Weinstein [1986].[1] Kelly acknowledges in his preface his indebtedness to this earlier work. The reason for mentioning the matter at this point is that the second central concept, a more natural and general one, that Kelly uses is that of learning. So his endeavour, like that of the earlier work mentioned, is to combine learning and computability. This kind of learning theory is sometimes called formal learning theory, but the description is not very adequate. It is the recursion-theoretic framework of formal leaning theory that Kelly uses extensively.

I should also say at once that the book contains no systematic scientific examples of induction in the sense of Kelly. The treatment is abstract except for anecdotal references to various kinds of scientific results, but none of those results are analysed in a standard way in any detail. Consequently, the distance between Kelly's theory and actual scientific practice is enormous. I also emphasize that this is not a point about which he is at all deceived. He is very straightforward in admitting this to be the case.

Aside from computability, there is another central and important idea at work in the book. This is that scientific methods should focus on guaranteeing eventual arrival at the truth. But this arrival at the truth works from the scientist's background assumptions. Kelly baptizes the pursuit of truth, in this sense, the *theory of logical reliability*.

[1] D N. Osherson, M Stob, and S. Weinstein (*eds*) [1986]: *Systems That Learn: An Introduction to Learning Theory for Cognitive and Computer Scientists*, Cambridge, MA, MIT Press

In various passages, Kelly refers to these background assumptions as given *a priori* in a particular inquiry. The way in which these matters are formulated gives a great many passages a highly Kantian flavour, which Kelly often explicitly emphasizes. Of course, because Kant does not discuss anything like computability concepts and their consequences, it would be better to say that Kelly expresses a neo-Kantian attitude in much of what he writes, especially in his rejection of purely empirical or probabilistic methods.

The book is well organized and, after two introductory chapters on philosophical perspective, presents a lot of interesting details according to the following outline. First, Chapters 3 to 8 cover the logic of hypothesis-assessment. This includes a formulation of the ideal logic (three chapters), Turing computability (two chapters), and a chapter on finite-state and complexity-bounded computations. Second, there are two chapters devoted to the logic of discovery, again beginning with the ideal analysis, that is, where everything works perfectly, and then a chapter on computable issues. Third, there is a chapter on the logic of prediction and, finally, there are four chapters giving various applications and generalizations. These include logic, probability, causation, and issues of relativism. It is a feature of the book that the latter chapters are much more sketchy and programmatic in character.

As my earlier remarks have already suggested, I am scarcely persuaded by Kelly's arguments that computability should play a role as important as probability in the development of scientific theories, hypothesis-testing and scientific inquiry generally. On the other hand, the book is full of interesting remarks and suggestive ideas. Many of the things that Kelly has to say provide a new perspective on old issues. The book is full of proofs, but many of them are relegated to the ends of chapters, so readers who want to get a philosophical and conceptual overview can do so without having to pursue all the proofs of theorems in detail. The proofs, in fact, often are taken from other sources, including Kelly's own published work. It is my impression that there are no theorems proved in the book for the first time that are of substantial depth and originality. This is not a criticism of the book. In fact it matches my own view as to how such things ought to be treated. Proofs of important theorems should first be given in the scientific journal literature and seldom, if ever, in books as such. On the other hand, it is desirable to have collected together in one place as many theorems and proofs as Kelly has. Certainly I found several results that I didn't know about and found significant. So again, the proofs are in no sense a waste. They are a desirable and integral part of the book.

Though different things are said in different places, I would take it that one of Kelly's basic ideas, very Kantian in spirit, is that the background assumptions a scientist brings to a particular investigation should be treated as *a priori*, and the logic of reliable inquiry be relativized to these background assumptions. This fundamental assumption on Kelly's part means that the investigation of a scientist's background assumptions, often considered an important part of scientific inquiry by other scientists, is not properly a part of his fundamental logic of inquiry. On the other hand, Kelly could well answer this remark of mine by saying that such assumptions can be investigated by another scientist who doesn't take them as part of his background assumptions and, for most purposes, this would be a reasonable way to think of the matter. Here is a good quote on this Kantian viewpoint towards the end of the book (p. 389): 'As always, my methodological stance is neither to accept nor to reject the scientist's assumptions, but to ask whether reliable induction is possible relative to them; even if they are transcendental.'

Let me end with a few critical remarks that do not diminish my respect and enthusiasm for the book. Disagreeing seriously with another philosopher is a standard way of complimenting his work.

1. Kelly's claims often sound extravagant in tone. Here is a typical quotation (p. 402): 'Philosophers of the inductive support stripe may complain that the results appear abstract and alien. But that is precisely because the results are logically grounded in the aim of reliably finding the truth, rather than being mere reflections of our social or psychological habits.' The contrast here between logic, on the one hand, and social or psychological habits, as the grouping for all other methods, seems extreme to say the least, and bold beyond what is possibly established in the book. Also interesting is the way in which Kelly wants to appropriate for this own methods the concept of reliability, in spite of its wide use in many parts of probability and statistics as a concept that has a precise and well-developed history in these domains. There is in his own work no serious analysis of this other tradition. In fact, because of its use of probability, he would, in his heart of hearts, like to be dismissive of it.
2. In a very wide range of probability circumstances, results that are shown to hold with probability one are considered, by most statisticians, to be as reliable or as close to certainty as one might expect in the real world. It is an important part of Kelly's methodology not to be satisfied with probability-one results and to give examples showing where sets of measure zero may have some role to play. So, he would like results proved for all possibilities, not just those comprising a set of measure one. But his desire to do so is not supported by any really interesting scientific examples and I am doubtful myself that any exist, even though I am sympathetic with a common Bayesian view that countable additivity of probability is not always desirable.
3. From a purely mathematical standpoint, there are some interesting remarks about why one is not always content to deal with probability-one results, why the real numbers are too lumpy in character, and why we

need something like infinitesimals properly to fill up gaps. But all of these relatively technical mathematical questions are certainly not shown to be of any interest whatsoever to physicists, let alone any other scientists. In fact, physicists mostly would consider such concerns as evidence of serious defects in methodology or as something to joke about, not to take seriously. I'm not suggesting by this that the physicists are always right, but just giving a sense of how far Kelly's discussion of methodology is from anything that goes on in any real empirical science, even one as sophisticated as physics.

4. Kelly has an unusual and idiosyncratic use of the term 'learning theorist'. As I noted above, the preface acknowledges the relation to formal learning theory, but the innocent philosophical reader could be lured into the mistaken view that formal learning theory is in the mainstream, rather than being, as it is, an interesting but minor current. The massive work on empirical theories of learning in psychology and education are of a very different kind, and anyone familiar with modern work in computer science and statistics on machine learning, with methods ranging from Bayesian statistics to neural networks, will know what a small part formal learning theory plays in that increasingly large literature.

The author expresses well, in a sentence on the penultimate page (p. 411), my own feelings about the book: 'The theory of computability is like a wonderful, infinite castle whose walls and hallways have been carefully mapped but whose tenants are still largely a mystery.'